

NOTE

ON

OVARIOTOMY AND OVARIAN TAPPING.¹

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In the *Lancet* of February 28, 1857, I published an article entitled, "Is Ovariotomy Justifiable?" I there showed that the arguments hitherto used in justification of this operation were either loose and illogical or absurd. In the *Lancet* of March 21, I find an answer to my paper by Professor Simpson, the gentleman whose arguments chiefly were impugned. My first impulse on perusing this paper was to lay it aside, as it contained much disagreeable matter, personal to me as a private individual, and nothing in the way of solid argument that demanded my attention; but further consideration has induced me to reply, even at the expense of some reiteration, and of the painfulness of controversy, however amiably conducted. The importance of the subject is an ample apology for this, and I shall avoid private matters carefully. I shall make my remarks under two heads.

I. WHAT DR SIMPSON HAS DONE.

1. He complains that I do him injustice in excluding ovariotomy from among those operations, of which he says that (to use his own words) "he particularly doubted whether surgeons were justified in so often subjecting patients to a great chance of speedy death, from a severe surgical operation, for the removal of a disease which might still allow of the continuance of life for many months or years," etc. Re-examination convinces me that I used this quotation justly. But if Dr Simpson even intended it to be used differently, I have no desire to cling to it. I never used it as an argument against ovariotomy, but only as an illustration of bad reasoning. He insists that these remarks do include ovariotomy—that is, his labours to justify ovariotomy leave him particularly doubting whether surgeons are justified in so often subjecting patients to a great chance of speedy death from it. A curious result. The qualification he introduces with the words "so often" cannot be important; for I suppose all

¹ From the *Lancet*, March 28, 1857.

will agree that an operation which is justifiable once is justifiable a thousand times, and the reverse.

2. Dr Simpson designates objections against the use of statistics in therapeutical questions, "common platitudes usually offered by young medical critics." I am already older than Newton was when he had completed many of his greatest discoveries. (Readers will pardon my presumption in introducing such a name into so small a question.) On the other hand, I think I have little more than half the number of years of Dr Alison, the most distinguished medical philosopher of this age, when writing the ominous warnings, designated as platitudes, etc., concerning the dangers of applying statistics to therapeutical questions. The age of an opponent is but a sorry argument. I am rapidly correcting this fault of mine.

3. Dr Simpson fixes upon amputation at the hip-joint as the arena on which to defend his statistics against my objection, that the circumstances of the cases included in the statistics were not known, so as to make us sure they had such reasonable amount of likeness to one another as to be fairly clubbed together. His defence consists in telling us only one circumstance, and that true, only of twenty-four out of eighty-four cases. The one circumstance is, that the amputations were for the cure of chronic disease. But this is evidently not even a good palliation of the error I point out. I have again to ask, what was the chronic disease? When an ovary is cut out, we have the well-known multilocular dropsy of it. But what is chronic disease? Is it cancerous, tubercular, or inflammatory, or all together? Is it of the bone, the joint, or of the whole limb? Alas! we are still in total darkness.

4. In concluding his remarks on cases of amputation at the hip-joint, Dr Simpson points out that the amputation was "fatal in the proportion of 75 in 100 cases in which it was adopted for the removal of hopeless chronic disease of the limb,—a mortality which is nearly double the mortality accompanying ovariotomy, when performed for the removal of hopeless chronic disease of the ovary." Here, it is evident, we have two defects, which vitiate the whole argument; for, we do not know on what grounds, if any, the cases are said to be hopeless, nor do we know whether the hopelessness was of life, or merely of cure of the disease.

5. Dr Simpson publishes a long defence of the absurd statement, that first tappings are fatal in one case out of every five operated on. It would be tedious to follow him through his argument, and useless; for my original article, already referred to, disposes of the whole matter in an irrefragable manner, and Dr Simpson's objections, so far from being at it, are scarcely beside it. For instance, he points out that a child passing through its sixth year has already passed through its first year, and adds something clever about a dull and dispassionate school-boy. He forgets that Dr Southam's table was first shown to be unworthy of any confidence in this question, and then that it is not one of first tappings, and that if a woman has been tapped twelve times, the first tapping could not have proved fatal. But such diversions from real argument are unworthy of the subject.

All Dr Simpson's argument, as to this point, is conducted as if he knew what use I would make of Southam's table, were it one of first tappings. I have not

entered upon the investigation of the real mortality of first tappings, and therefore have never indicated what use I would make of it. I shall only say, I will never use it in the absurd way he too early attributes to me. But in his defence of his own use of it he has fallen into an error of reasoning, which, in deference to its author, I shall not class among the platitudes, etc. The error or fallacy consists in passing off what is true abstractly, as if it were true in its relations to the question in hand. Thus, it is most palpably true that the sixteen cases in Southam's table, which, according to himself, had undergone tapping once or several times without dying from it, must all have been tapped once safely. But this very simple truth, as to sixteen cases surviving, has no proportional relation whatever to the other four that died, in respect of the question of the mortality of first tappings.

To use Dr Simpson's own example. What would Farre or Neison think of the mortality of the first year of infancy being arrived at by selecting four cases dying in that year, and sixteen who survived it? It would be true that four died in the first year, and that sixteen did not; but these numerical facts would have no numerical relation to the question.

After all, will Dr Simpson, or any one else, really attempt to defend Southam's table, made up as it is from a set of cases selected to illustrate the course, and especially the terminations, of ovarian disease, when used as a means of ascertaining the fatality of first tappings? Again, will Dr Simpson, or any one, defend the proposition, that first tappings are fatal once in every five cases? I may remark that I lately received a note from one of the most distinguished, oldest, and certainly of the most experienced obstetricians in the three kingdoms, informing me that he had never seen a death from tapping at all. Many others have told me the same.

II. WHAT DR SIMPSON HAS NOT DONE.

On this subject, I cannot venture to occupy the valuable columns of this journal, further than to say that my whole original article, referred to, remains unanswered. I am quite willing it should stand without further defence, as it does not need it. I beg those interested in the question only to read it dispassionately.

There is one aspect of Dr Simpson's paper which I regret having even to notice. This is the attempt made, directly and indirectly, to injure my private character. I am described by Dr Simpson as having "totally misrepresented, in various respects, the statements" of himself and others; next, it is said that I only "misrepresent some of the opinions and statements" of Dr Simpson; then it is proposed to "show the various forms of misstatement and error which he (Dr D.) has committed;" then it is asserted, in regard to a statement of mine, "that the exact reverse of this is true," and it is implied, if not said, that I knew this to be the case; then it is said that "Dr Duncan is entirely wrong when he makes these very characteristic observations," and it is added, "'Nothing,' long ago observed Dr South, 'is so haughty and assuming as ignorance, when self-conceit bids it set up for infallible.'" Then it is tried to cast ridicule on me for speaking even hypothetically of gangrene of a limb

after fever, requiring amputation; an operation which I have seen performed by a distinguished hospital surgeon. Further, it is said by Dr Simpson that he "believes that no medical man, except Dr Duncan . . . can have any doubt in his mind that they were instances in which the first operation of paracentesis was the more immediate source of death of the patient, by exciting peritoneal inflammation." These words, "except Dr D.," etc., were written by Dr Simpson with the following words of mine under his eye. My words are,—Case 1. *This is evidently an example of death from tapping.* Case 2. *This case is also selected in order to illustrate death from tapping.* Case 3. *Death was the result of the first of an intended series of tappings.* In my account of the fourth case, I refer, without hesitation as to the result, to my original paper. Again, Dr Simpson attributes the antithesis of the dangers of phlebotomy and tapping, made use of by M. Velpeau, to me. Again, he says, "It is really difficult to comment upon such sad affirmations and unhappy reasonings as these with due and proper gravity." And again, he adds, "If Dr Duncan's communication had been confined to our own society or journal in Edinburgh, where he is sufficiently known, I would, in all probability, not have taken the trouble of publishing any correction of his misstatements," etc., etc.

Such are some of the statements in Dr Simpson's paper on Ovariotomy and Ovarian Tapping. Are they arguments? No. Personal abuse is known to be a resort of those who feel the deficiency of good argument.—Are they worthy of professional writing or debate? No.—Are they palliated by Dr S.'s position in the profession, while I am perhaps still included among the juniors? No.—Did I give Dr S. any example of such writing or debate? No. I can truly say, that while I hesitate not to call arguments or measures illogical, absurd, or ridiculous, when they so appear to me, I have never, in any publication, anonymous or otherwise, stooped to characterize an author or an opponent in the manner which Dr Simpson, so unprovokedly, adopts towards me. Neither shall I so far belie my own reputation as to seek now to defend it. I appeal to the profession, without defence and without reserve. And I shall not attempt the disgusting task of applying similar treatment to my opponent. I am proud to say, that, so far as I know, the mass of the profession is on my side. Since my paper was published, I have received numerous and valued testimonies in its favour from my professional brethren. Several of these are from most distinguished, if not the most distinguished obstetricians, physicians, and surgeons in London, Dublin, and Edinburgh. Their words are often too flattering to me, and, of course, not intended for publication. But they have been the greatest possible comfort to me in entering, even in the careful way I did, into the troubles of controversy.

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